PEARL

The Journal of HEREDITY

IDENTICAL IN ALL BUT TASTE ACUITY
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VOLUME 27 DECEMBER, 1936 NUMBER 12

The American Genetic Association

An incorporated organization devoted to promoting a knowledge of the laws of heredity and their application to the improvement of plants, animals, and human racial stocks

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The JOURNAL OF HEREDITY is published monthly by the AMERICAN GENETIC ASSOCIATION at 32nd Street and Elm Avenue, Baltimore, Md., and Victor Building, Washington, D. C. Address all communications regarding editorial matters to Room 308, Victor Building, Washington, D. C. Communications regarding the business management of The JOURNAL OF HEREDITY may be addressed to the publication office, 32nd Street and Elm Avenue, Baltimore, Md., or to the Victor Building, Washington, D. C. Entered as second-class matter February 24, 1923, at the postoffice at Baltimore, Maryland. Acceptance for mailing at the special rate of postage provided for in section 1103, Act of October 3, 1917, authorized December 20, 1918.

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$3.00 a Year. 35c the Copy. Prices of back numbers on application.

The

Journal of Heredity

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SPIRAL STRUCTURE OF CHROMOSOMES

Frontispiece

Chromosomes of *Vicia faba* from a metaphase plate in a somatic cell (8,500×), showing the helical chromonemata which they contain. The *M* chromosome is included in the photograph and shows the spindle fiber attachment region in the upper right hand corner and the secondary constriction in the left arm. In the distal portion of the right arm four helically coiled chromonemata (H_1) may be observed. Beginning at the right one may see the coils of one chromonema in optical section, as a series of almost equally spaced dots (*A*). The next chromonema to the left is seen in focus, the left edge of a portion of this helix being outlined with a white line (*B*). The two other chromonemata on the left are somewhat out of focus. Compare this figure with Figure 1*B*, which is the same chromosome at a slightly different focus. There the first helix on the right is in focus. In examining 1*B*, the reader may find it advantageous to use a hand lens.

THE STRUCTURE OF SOMATIC CHROMOSOMES

A. MARSHAK

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THERE are at present two apparently conflicting theories concerning the structure of somatic chromosomes. One supported by Kuwada and Darlington maintains that each anaphase chromosome contains a single helically coiled chromatic thread, the chromonema. The other, based on descriptions given by Kaufmann, Sharp, Koshy and others requires the presence of two interlacing chromatic threads in each anaphase chromosome. Darlington has pointed out that in the latter case the plane separating the two chromonemata would cut across the coils of the helix. It would seem, therefore, that the two theories were mutually exclusive. However, the findings of this investigation make it possible to reconcile the differences between these two concepts of chromosome structure and indicate that each of them is true only in part.

Chromosomes of the following species have been examined:—*Allium cepa, Allium porrum, Lactuca sativa, Papaver orientale, Pisum sativum,* and *Vicia faba.* They all show essentially the same structure. A smear technique was used for all preparations, which will be described in a subsequent more detailed report of these experiments.

At metaphase each chromosome contains four chromonemata, each of which pursues a remarkably regular helical path. This type of fixed chromosome will be designated as fixation image *a.* The smallest visible helix will be referred to as H_1, the dimensions of which are given in the following table:

The length of the chromonema may be calculated from the equation:

$$S = \sqrt{a^2 + b^2} \cdot 2\pi n$$

where S = the length of the helix
a = the radius of the helix
b = the pitch of the helix
n = the number of revolutions

The value n is obtained by dividing b into the sum of the lengths of all the chromosomes L, L being measured from photographs or camera lucida drawings of suitably oriented metaphase plates.

The direction of coiling of H_1 was determined in 26 anaphase chromosomes of Vicia. Of these, 8 were clockwise and 18 counterclockwise. In every case the members of the pair of H_1 in each anaphase chromosome were coiled in the same direction.

The cylinders about which the H_1 systems are wound also pursue a more or less helical path, H_2, of larger diameter than H_1. In Vicia the H_2 helix may show but one or two revolutions. It was possible to determine the direction of coiling of both H_1 and H_2 in 8 chromosomes and in all cases the direction of coiling of H_2 was opposite to that of H_1.

At telophase the pairs of H_1 remain distinct and apparently entirely separate from one another except at the spindle fiber attachment region. In x-rayed material where both members of the disjoining pairs have become terminally attached, there is no evidence of the pairs of H_1 becoming fitted into each other to form a single helix with a longitudinal split or fissure. This behavior of the H_1 helices is also evident in chromosome

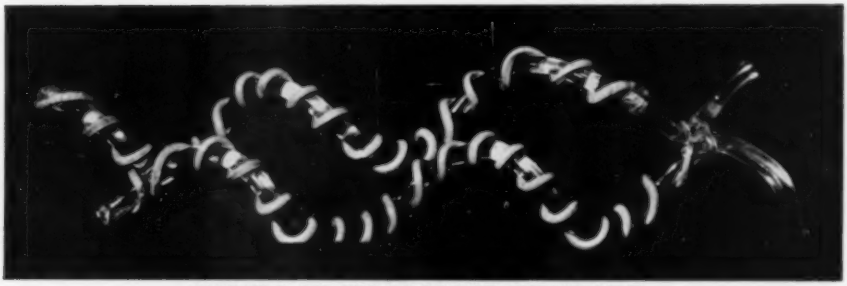
DIMENSIONS OF H_1

Species	Stage	$2a \times 10^{-4}$ cm.	$b \times 10^{-4}$ cm.	$L \times 10^{-4}$ cm.	S cm.
Pisum sativum	*metaphase*	0.48	0.72	1.295-1.35	0.0995-0.104
Vicia faba	*metaphase*	1.03	0.70	2.50-2.52	0.195-0.196
Allium cepa	*metaphase*	0.70	0.63	3.86 (approx.)	0.277 (approx.)
Vicia faba	*resting*	0.74	0.70	———	———
Vicia faba	*mid-prophase*	0.74	0.41	———	———

HELICAL CHROMONEMATA

Figure 1

See legend at bottom of opposite page.



MODELS OF CHROMOSOME STRUCTURE

Figure 2

Model illustrating the structure of a portion of a chromosome in somatic anaphase. The glass helices represent the H_2 systems and the electric cords wound in helical form about the glass helices represent the systems designated as H_1. The glass helices were made by winding glass rod about heated iron pipe. On a smaller scale, very satisfactory models may be made with pipe cleaners which can be wound about a pencil. They may be stained with alcoholic solutions of analine dyes like gentian violet or eosin to distinguish the different systems. The reader is advised to use such models as aids to the visualization of the theories of chromosome structure.

fragments produced by x-rays. Such fragments may occur singly, or in pairs, in the latter case representing homologous portions of the disjoining chromosomes, as may be determined from their similarity in size and morphology. In the telophase, single fragments become more or less spherical and contain a non-staining hyaline material. The H_1 helices are found at the periphery, usually at opposite poles of the sphere. Pairs of fragments, show four independent helices at telophase.

During the interphase (resting stage), the H_1 helices remain independent. There are no anastomoses or lateral branches. The helices are regularly coiled with no evidence of straightening out or uncoiling during the telophase-interphase interval. The pitch of the H_1 helix of *Vicia faba* is 7×10^{-5} cm. at metaphase and in the resting stage. The helices are radially arranged about a region presumably representing the pole of the spindle of the previous anaphase. Additional coiling of the H_2 systems ("supercoils") does not appear until prophase. This may be readily determined by examining cells which will no longer divide, such as epidermal cells and the elongated cells of the vascular system.

At the onset of prophase, there are pairs of laterally associated H_1 helices instead of the single helices of the interphase. These are coiled about one another to form the H_2 helices and the latter are thrown into additional bends or loops ("supercoils"). As the prophase proceeds the H_1 coils appear to increase

HELICAL CHROMONEMATA

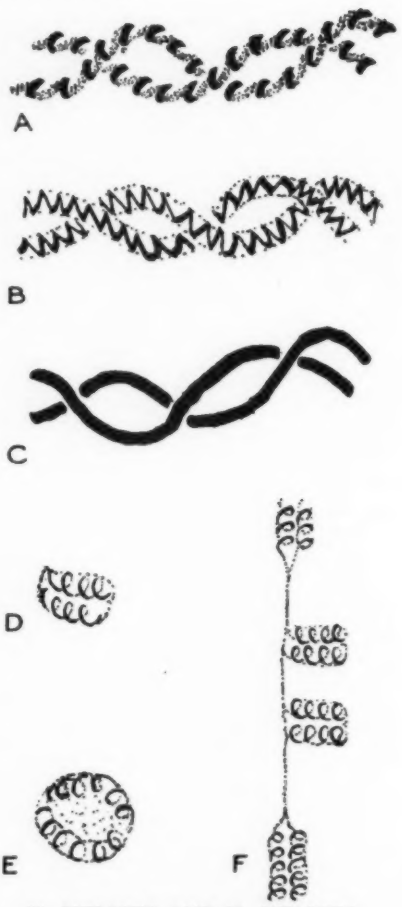
Figure 1

Somatic chromosomes of broad-bean, onion and pea (all magnified 3,200×) showing the helical structure of the chromonemata which they contain. *A—Vicia faba* anaphase. Two helical chromonemata may be seen in each anaphase chromosome. These pairs of H_1 helices wind about each other along helical paths of larger diameter, the H_2 helices. *B—Vicia faba* metaphase. Slightly different focus of the same chromosome as in Frontispiece. In the lower right arm the first chromonema to the right is now in focus. *C—Vicia faba* resting stage. A helical chromonema is in focus at about the center of the nucleus. Most of the others are fore-shortened by perspective so that turns of the helix may be mistaken for chromomeres. *D—Vicia faba* mid-prophase. The arrow indicates the end of a chromosome which is in focus and shows only two helical chromonemata. *E—Allium cepa* metaphase. The end of a chromosome showing the four chromonemata. *F—Vicia faba* metaphase. The end of a chromosome showing four chromonemata. *G—Pisum sativum* metaphase. Three helical chromonemata may be seen. The fourth is out of focus.

in diameter. The H_1 diameter of *Vicia faba* at mid prophase is 7.4×10^{-5} cm. as compared with a similar metaphase diameter of 11×10^{-5} cm. However, the pitch of the H_1 helices is 4×10^{-5} cm. at mid-prophase. This can only be explained as an increase in the intensity of coiling of the original pair of H_1 which were discernible at the previous metaphase, since the new chromonemata are not yet visible. Kuwada[5] has ascribed the greater intensity of coiling to the new H_1 helices that he believes are formed at this time. The significance of the present finding will be discussed elsewhere.

Slight modification of the fixation technique results in collapse of the cylinders about which the H_1 helix is wound. The latter then appears as a zigzag thread (fixation image *b*). Carried to an extreme, all evidence of H_1 may be lost, with only the H_2 systems remaining as flat ribbons or threads (fixation image *c*). Modification in another direction produces chromosomes as solid cylinders with no evidence of either H_1 or H_2 except occasionally at the distal ends where an apparent split may be seen, representing the space or material between the H_2 systems.

Fixation image *c* where each anaphase chromosome contains a pair of interlacing threads or ribbons is the condition described by De Horne[2], Kaufmann[3], Sharp[5], Koshy,[4] and others. Kuwada[6] considers that each telophase chromosome contains a helical chromonema which is longitudinally split, not two independently coiled chromonemata as are described here. Upcott[7] has described and photographed fixation image *a* in pollen-tube divisions of *Tulipa Greigi*. However, she and Darlington[1] have observed only one helix in each anaphase chromosome and have pointed out that the plane of separation as described in the "interlacing" type of chromonemata would cut across the turns of the helix. But there are two such helices in the anaphase chromosome and these coil about each other in such a manner that when upon alteration of the fixation procedure the H_1 helices are obliterated, the result is the apparent interlacing of the



H_1 HELICES AND H_2 HELICES DIAGRAMMED

Figure 3

A—Fixation image *a*. The helical form of the pair of chromonemata in each chromosome is preserved. H_1 and H_2 are both discernible. *B*—Fixation image *b*. The H_1 helices are collapsed but are still differentiated. *C*—Fixation image *c*. The H_1 helices are obliterated. Only the H_2 helices can be seen. *D*—A single chromosome fragment in anaphase produced by irradiation of the preceding resting stage. Both H_1 helices are clearly defined. *E*—A similar fragment in telophase. The helices are at the periphery of a spherule that resembles a minute nucleus. Note that the H_1 helices are now widely separated from one another. *F*—An attachment between a pair of anaphase chromosomes (irradiated with X-rays in the preceding prophase) breaking to leave a pair of fragments.

H_2 helices. Thus the two contending hypotheses concerning chromosome structure may be attributed to the use of only one type of fixation on the one hand, and inadequate observation on the other.

In accordance with an earlier suggestion by Nebel, Darlington and Upcott consider that there is no matrix material in the chromosome other than that which may be imagined as a coating about the chromonema. The rubber insulation about the copper wire of an electrical cord will serve as an illustration. However, it seems evident from the helical nature of the chromonema that there must be some internal substance. One cannot coil a thread about a vacuum. The marked expansion of this substance in acids or alkalies, and the collapse of the chromonema after certain fixation techniques suggesting solution of this material may be a clue to its chemical structure. Since existing evidence concerning the nature of the matrix is scanty to say the least, lengthy discussions about it may profitably be avoided. However, the problem itself cannot be brushed aside simply by being ignored, for it seems very likely that the structure of the chromonema itself will not be fully understood until its relations to the materials immediately surrounding it are determined. And such knowledge would seem to be essential to the complete understanding of genic determination of development, for the chromonema and matrix represent the milieu in which the genes and their immediate products function.

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✦

FACTS AND FEELINGS IN SCIENTIFIC WRITING

To the Editor:

Out of the twelve columns of Mr. Cook's review of the cancer chapter of *Outposts of Science* which appeared in the September, 1936, issue of this Journal the following three charges can be distilled:

(1) Too much sugar-coating, the "circus barker's appeal to hooey and magic and the gibberish of a babe in the woods."

(2) A biased account because of the setting up of Maud Slye on a pedestal and ignoring the work of others in the field.

(3) Some conclusions unwarranted or altogether inaccurate.

All of these criticisms are unjust because:

(1) The verdict of the press, both scientific and lay, refutes the first charge. Note: *Edwin G. Conklin,* "I have learned a good many things that are of interest in fields with which I am especially acquainted, and I congratulate the author on having made so thorough a study." *Quarterly Review of Biology,* "Authoritative. Should be read by professional and lay persons alike." *Industrial and Engineering Chemistry,* "Maintains the high level of humanizing science which won him the Francis Bacon Award." *Waldemar Kaempffert* in *New York Times,* "Solid information. It stands as the most admirable presentation of latter-day science which ought to be required reading in every college." *Harry Hansen,* literary editor of *Harpers,* "A clear narrative that never comes down to an inferior level. It is the best kind of popular writing."

(2) The subtitle of my cancer chapter admits that I have told my story chiefly around the work of one personality. This is also true in some measure of all the other chapters. The scope of the undertaking compelled such a method of treatment. "This will arouse," writes Mr. Cook, "feeling of irritation and resentment in the breasts of many other workers." I doubt it: no other reviewer or researcher complained. And even though the

reviewer admits that he "is not in touch with the situation on much of the cancer front," I shall be glad to consider the work of any of his outstanding workers in the cancer field whom I have neglected and mention their contributions in a future revision of Outposts.

(3) The inheritance of cancer in all its manifold details is still not settled. "There is but one right," said Huxley, "and the possibilities of wrong are infinite." Still I accepted the conclusions of the courageous Maud Slye because they were bolstered up by Felix Bernstein, one of the world's most eminent mathematical statisticians as well as by the judgements of others. Dr. C. C. Little, the stormy petrol in the controversy over the inheritability of cancer, has been the outspoken opponent of Maud Slye for a generation. His most recent flareup was a challenge to Dr. Slye to loan some neutral institution for breeding purposes one thousand of her male mice which, she would certify, as having no tendency to cancer. He would give one thousand of his cancer-susceptible female mice for breeding brother to sister. *Time* (Nov. 16, 1936) quotes Maude Slye's answer as follows: "The non-tumor strains with which this preliminary work was done are now no longer in existence. Some five years ago I offered to make this test. I drew up a contract for this experiment and submitted it to Dr. Little. To this offer I received no reply and the matter was dropped."

Since Mr. Cook's review was written in very bad taste, I cannot feel offended. But it is a pity that the editor of a respected scientific publication had to sink to the low level of unkindness in attacking not only Maud Slye's knowledge of the laws of genetics but even of her poetic expressions. What chivalry towards a woman who is giving a life to scientific research.

One almost loses faith in those scientific attitudes which are said to identify the man of science.

BERNARD JAFFE

Nov. 24, 1936.

IF THE review which Mr. Jaffe complained of exceeds the bounds of good taste the reviewer naturally regrets that this should be so. He is not certain however that he is the first to sin in this respect. It should be clear that the review was written more because of the very favorable reception the book received than because of its inherent faults. Many books and articles are published which do violence to genetic and biological concepts, and it would be impractical to give adequate attention to all of them. When serious errors of fact and interpretation are given a rousing reception, the situation is materially changed and it seems almost a duty to "note an exception," as the lawyers say. If the errors cited in the review are in fact errors (and Mr. Jaffe makes no defense of them) then the criticisms there made would seem to stand, and the question of scientific good taste would partly be shifted back to the author. Several errors of genetic fact and interpretation were intentionally given in the review in enough detail so that any intelligent person could check them.

It is obvious that the errors discussed in the review are not merely of theoretical implication, but are of a nature to exert considerable influence on medical and lay attitudes with regard to the cause and control of cancer. To amplify one example: If, as Mr. Jaffe alleges, cancer is controlled by a single recessive gene, and if vital statistics are correct in attributing to this disease more deaths than to any other single cause, it follows as a necessary corollary that the hypothetical "recessive cancer gene" must be almost universally disseminated in the population. If this were true, the picture would be dark indeed, and eugenic elimination of cancer would be hopeless, because it would be necessary to sterilize or otherwise withhold from reproduction a very large part of the population, possibly as much as three-quarters. Were our "cancer gene" a dominant its elimination would be relatively very much simpler. Under any conditions the picture has a distinctly somber cast, and the absurdity of alleging that the most discouraging view is a cheerful picture is obvious. The facts being as they are, it would appear that a testimonial from Father Mendel himself would hardly "whitewash" the author's genetics. The fact that so eminent an authority as Dr. Conklin praised the book would merely imply that he had not read all of the book with a critical eye.

The "Bernstein angle" and the recent very interesting note in *Time* may be disposed of together. The difficulty of applying statistics to Miss Syle's material, or to that of the other cancer investigators whose material Bernstein used, is that pure-breeding strains of mice with respect to cancer incidence had not been established before the crosses were made. Thus Bernstein's confirmation was necessarily very much of a "bolstering up." Assuming that the "foundation animals" were of the postulated genetic constitution, a single gene explanation is *possible*. The difficulty is that the primary assumption regarding the genetic constitution of these dead animals could never be verified, and other assumptions are equally possible. Somewhat similar considerations are involved in Miss Slye's reply to Dr. Little as reported by

(*Continued on page* 472)

THE DIAGNOSIS OF ZYGOSITY IN TWINS

Three Instances of Differences in Taste Acuity in Identical Twins

S. N. Ardashnikov, E. A. Lichtenstein, R. P. Martynova, G. V. Soboleva and E. N. Postnikova

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IDENTICAL TWINS DIFFERING IN TASTE ACUITY

Figure 4

These twin girls on the basis of similarity in many characteristics would be classified as identical twins. Nevertheless they differ in taste reaction to phenyl-thion-carbamide (P.T.C.). In a series of 137 pairs of twins classified as identical three pairs differed in their reactions to P.T.C. The conclusion that would seem to be drawn from this is that the expression of the gene determining taste acuity to this substance is somewhat variable.

THE common method of diagnosis of twin zygosity depends on analysis of "external similarity" (Siemens, Verschuer; also Bunak, Rife and others). The present investigation is an attempt to determine the accuracy and efficiency of this method. Seventeen morphological traits were employed in determining zygosity (see Table I). At the suggestion of Levit, the blood groups and the taste reaction for phenyl-thio-carbamide (designated RPTC) were also checked against the above scheme.

Method and Material

Of the 1,035 pairs of twins available at the Institute 441 pairs (292 of like and 149 of unlike sex) were examined by means of the above scheme. Only twins over six months of age were used, since the traits are not clearly manifested and cannot be accurately estimated at an earlier age.

TWO OTHER TWIN PAIRS DIFFERING IN TASTE ACUITY

Figure 5

Of the 137 pairs of identical twins examined, these two pairs of boys and the girls illustrated in Figure 4 were the only ones which differed in ability to taste phenyl-thiocarbamide (P.T.C.) In external appearance, as shown in the photographs, and in many measurements these twins were unequivocally classified as identical. It would seem therefore that these differences in taste acuity can hardly be explained as being due to the subjects being in fact fraternal twins, and not identical.

Classification

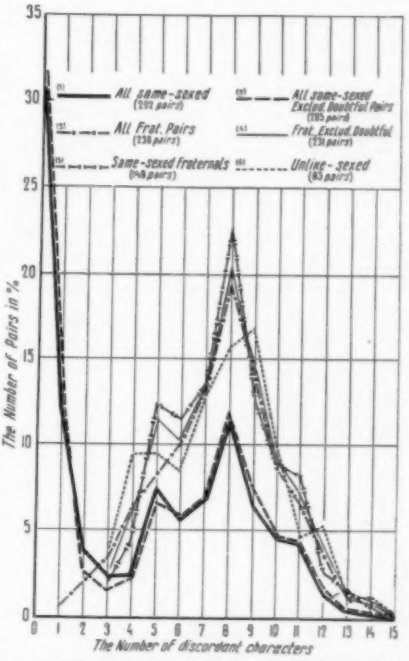
Figure 6 gives the distribution of the twins according to the number of traits for which there were intrapair differences (independent of the degree of these differences). This distribution of intrapair differences shows that, in the main, our material consisted of two distinct groups. Pairs in which the members differed in no more than two traits and not in more than one graded interval into which each of the traits were subdivided, we considered to be monozygotic. Of the like-sexed twins 137 (46.92%) were so classified on the basis of these two criteria. Of the remaining 155 pairs, 148 (50.68%) fulfilled neither of these two criteria and so were classified as fraternal. Seven pairs (2.4%) fulfilled one of the two criteria, usually the first, and were classified as "doubtful." These criteria for classification were based upon probabilities of complete similarity or difference in one, two, or more traits in a pair of identical twins. These estimates may be obtained from the data presented in Table II.

It is evident from the above table that intrapair similarity in the fraternals differs significantly from the intrapair similarity in the identicals, the doubtful twins occupying an intermediate position. This shows that these traits (see Table I) may be used in making diagnosis of zygosity, the various traits having different significance in the general scheme (see Table II).

Errors in classifying twins may be of two kinds. They may arise either from an inaccurate inclusion of identical twins in the group of fraternal, or vice versa.

Erroneous Classification of Identical Twins as Fraternals

If there are many such errors, the average intrapair similarity for hereditary traits among fraternal twins would be greater than the corresponding theoretical expectancy. The same would be true if cases of "one and one-half zygosity" (Dahlberg, Kurtius, Verschuer), if such exist, were included in the group of fraternal twins.



CONCORDANCE AND DISCORDANCE IN TWIN RESEMBLANCES

Figure 6

The 441 pairs of twins utilized in this study fell into two clearly defined groups, with respect to intra-pair resemblances. Very few cases fall into the doubtful class in a system of diagnosis based on intra-pair resemblances in many characters.

To determine whether such errors were present in our material the distribution of fraternal twins was compared with an equivalent sample from a theoretical sib population. Sib association tables were calculated for blood groups, RPTC and for both together. Since complete information about the gene constitution of the parent population was lacking, we deduced it from the gene constitution of the twin population.

It is evident from Table III that there is no significant deviation in the group of fraternal twins from a distribution of sibling pairs. We can thus assume that if some identical twins are incorrectly classified as fraternal, the

error would be too small to affect the general character of the group. Similarly, errors arising from the inclusion of "one and one-half zygotic" twins are in reality too small to affect the general classification (Levit and Soboleva).

Erroneous Inclusion of Fraternal Twins with Identical Twnis

The erroneous inclusion of fraternal twins with identical apparently seldom occurs since their genetic intrapair differences would be apparent at least in some cases for such traits as RPTC-acuity or blood groups. In our material there were intrapair differences for RPTC-acuity in three pairs. The members of the first two pairs (No. 762, Figure 5*A*, and pair No. 610, Figure 5*B*) showed complete similarity for all other traits. The members of the third pair (No. 1005, Figure 4) seemed upon superficial examination to be very different, particularly because one of the members was pathologically handicapped. As a matter of fact the partners of this pair differed in only one trait (thickness of lips). Intrapair difference for fingerprints in each of the three pairs was comparatively small.

These doubtful pairs were examined several times and conferences were held to finally decide the zygosity. On the basis of all the data they were classed as identical twins. Intrapair differences for RPTC were considered as due to phenotype variation of the gene for RPTC.

Table I. Characters Investigated for Diagnosis

Diagnostic characters	Number of distinguishable forms	Variations
1. Eye color	12	Scheue of Bunak
2. Eye form	3	1-Small 2-Intermediate 3-Large
3. Size of eyebrow	3	1-Slight 2-Intermediate 3-Heavy
4. Form of eyebrow	3	1-Oblique 2-Straight 3-Curved
5. Length of eyelash	3	1-Short 2-Intermediate 3-Long
6. Form of eyelash	3	1-Straight 2-Slightly curled 3-Very much curled Scale proposed by Fisher
7. Hair color	27	
8. Form of Hair	3	1-Straight 2-Wavy 3-Curled
9. Bridge of nose	3	1-Low 2-Intermediate 3-High
10. Nose profile	3	1-Concave 2-Straight 3-Convex
11. Base of nose	3	1-Raised 2-Straight 3-Grouped
12. Tip of nose	3	1-Raised 2-Straight 3-Grouped
13. Width of lips	3	1-Very thin 2-Thin 3-Intermediate 4-Thick 5-Very thick
14. Size of mouth	3	1-Small 2-Intermediate 3-Large
15. Helix of ear	3	1-Few folds 2-Intermediate 3-Many folds
16. Anthelix	3	1-Slight 2-Intermediate 3-Pronounced
17. Lobe of ear	3	1-Grown fast to cheek 2-Half free 3-Free

Table II. Intrapair Variability in Diagnostic Characters

	Difference							
	Identical twins		Zygosity unknown		Same sex fraternal twins		Unlike sex fraternal twins	
	Number	%	Number	%	Number	%	Number	%
Eye color	12	4.0	3	43	193	79.1	104	77.6
Eye form	6	2.0	5	71.4	107	51.9	49	40.8
Size of eyebrow	3	2.5	0	0	79	39.7	47	39.4
Form of eyebrow	6	1.7	3	43	43	43.8	35	40.7
Length of eyelash	0	0	0	0	73	49.3	40	45.5
Form of eyelash	0	0	2	28.6	43	30.8	29	31.8
Color of hair	20	6.9	6	33.3*	174	62.9	79	61.4
Form of hair	1	0.3	0	0	84	36.8	25	28.5
Bridge of nose	5	1.4	3	4.3	-	28.9	38	31.9
Nose profile	8	3.2	1	14.3	88	34.7	39	39.3
Base of nose	9	3.5	0	0	90	36.4	51	38.6
Tip of nose	13	6.1	1	14.3	83	39.0	47	39.8
Size of mouth	6	2.9	1	14.3	91	41.7	49	41.9
Width of lips	9	3.6	1	14.3	109	44.5	60	43.5
Helix of ear	5	2.3	1	14.3	60	29.8	40	41.7
Anthelix	3	2.3	0	0*	135	39.4	43	43.3
Lobe of ear	13	5.6	3	43	113	48.3	38	43.7

*This character was not investigated in one pair of twins.

Table III. Probability that the Distribution of Fraternal Twins Corresponds to a Sib Population

			Like sex fraternal twins		All fraternal twins	
			χ^2	P	χ^2	P
Blood group	Russians	4	2.024	0.67	2.677	0.62
	All twins	4	1.089	0.89	2.353	0.64
RPTC	Russians	1	0.174	0.68	0.936	0.33
	All twins	1	0.566	0.45	2.302	0.13
Blood groups and RPTC	Russians	5	0.899	0.9398	1.830	0.61
	All twins	5	0.739	0.86	2.152	0.54

Conclusions

1. The diagnosis of twin zygosity based upon the method of external similarity is satisfactory for widespread investigations.
2. We found no evidence for "one and one-half zygosity."
3. The gene for RPTC seems to be phenotypically variable in some cases.

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MOTIVATION OF CHILDLESS MARRIAGES

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THE increasing proportion of childless marriages has been a striking feature in America as in other civilized countries. Figures show that the proportion of marriages in the United States that never produce a child is about 17 per cent, while if consideration is limited to the educated part of the white population, the amount of childlessness is nearer one in five.

Some of this childlessness is voluntary, some involuntary, and there have been wide differences of opinion as to the proportions of the two kinds in the total. Analysis of childless marriages, in the data collected by Katherine Bement Davis, showed that three-fourths had wanted children and one-fourth were childless by preference. It is not certain, however, even when people are answering an anonymous and confidential questionnaire, that they will tell the whole truth in this connection. It is easy for them to rationalize their own feelings. In addition to the explanations of the childless couples themselves, there is need of a commentary from their close friends and relatives, whose interpretation of the reasons for a childless marriage might not wholly correspond with the interpretation given by the principals.

I therefore asked more than 100 adult students at the University College, University of Southern California, to list all the cases of permanent childlessness that they knew among their closest friends and relatives, selecting only such marriages as they felt sure would at no time in the future produce children, and only those couples whom they knew so intimately that they felt no doubt as to the motivation of childlessness. The students contributed histories of 862 couples, which are nearly all from the educated part of the population, with an excess of professional people, particularly teachers. The findings might not apply to a wholly different socio-economic level of the population. How far they are typical of the white-collar population in other states than California is a matter of opinion and, I hope, of investigation by others in the same way.

These were divided as follows:

Voluntary	582	67%
Involuntary	280	33%
	862	100%

It will be noted that in only one-third of these marriages was childlessness undesired, as compared with three-fourths in K. B. Davis' study. One can not be sure that this percentage is representative. In the first place, my students may unconsciously have selected cases in which they knew the couple did not want children. In the second place, the percentage of biological sterility probably varies greatly in different parts of the United States and in different social strata. Personally, I do not believe that the finding of Dr. Davis (three-fourths of the childless marriages involuntary) is representative of the facts among educated people at the present day; but this is a question that deserves further study.

The causes of involuntary sterility can not be studied profitably by the method here used. Often the causes are not known even to the subjects themselves. Some childless marriages in this series are due to the advanced age of the wife at marriage. More than 10 per cent are due to surgical operations performed on the wife (or occasionally the husband) which resulted either incidentally or intentionally in sterility. In another 20 per cent of cases, some disease or other physical condition was thought to explain childlessness. But all these factors together leave nearly two-thirds of the cases in which my informants at least had no knowledge of

the causation. It would not be profitable to analyze any further the cases of involuntary sterility in this series.

The detailed enumeration of presumed reasons for voluntary refusal to bear children was extremely interesting, but difficult to summarize satisfactorily. For the present purpose they may be classified as follows:

	Number	*Percent*
Self-centered	180	31
Wife's career	128	22
Economic pressure	96	16
Health	51	9
Dislike of all children	49	8
Miscellaneous	36	6
Eugenics	27	5
Marital discord	15	3
	582	100

1. "Self-centered" is a term I have used, for lack of anything better, to cover such comments as "social climbers," "wanted to be free to travel," and the like. Mr. and Mrs. *A.* are "too busy working and making money to bother with children." In the case of the *B.'s*, "wife wants to teach; husband wants to hunt and fish; each wants to follow own inclination and a child would disturb both of them." The history of the *C.'s* is a common type: "they wanted to save for a home and furniture first, but never reached the point where they were satisfied." The *D.'s* think an apartment is no place to bring up a child; and they simply couldn't consider living anywhere except in an apartment! Mrs. *E.* can't afford a maid, and she is certainly not going to let a child interfere with her club and social activities. Such histories are typical of a large number.

Perhaps a dozen of the wives have avoided pregnancy because, as they frankly told their friends, they feared the experience would spoil their looks or figures, and they did not want to make that supreme sacrifice. They are well matched by an equal number of men who insisted on sterility because they were afraid a child might take first place in the wife's affections, leaving them to "eat at the second table" of affection, without the amount of petting, mothering, and babying that they craved. Four couples had foregone parenthood, my students said, because this feeling was mutual: "they were so much in love with each other that they couldn't bear to think of children that might come between them and spoil the perfection of their romance." This is the much advertised Companionate Marriage raised to the *n*th degree!

2. "Wife's career" covers those cases in which the wife gave up maternity to work, not because she needed the money but because she preferred the outside occupation and did not want to interrupt it. This classification is large because so many of my students are teachers, social workers, and the like, and report the cases of their own friends in the professions.

Mrs. *F.* "doesn't like to stay at home," so she works instead. Mrs. *G.* has a little better excuse,—she has taught for 20 years and has not only "got the habit" but does not want to lose her retirement pay, which will soon be due her. Most of the stories under this heading are of a commonplace type: the wife was educated for a career, not for motherhood, and she wants her "freedom."

3. "Economic pressure" is a classification intended to take only those cases where husband and wife could not afford children,—not those in which they preferred to be childless and rationalized this emotional attitude by explaining that they didn't want to have a child until they were sure they could give him every advantage, etc., etc. Doubtless rationalization enters into a good many of the cases in this category; but some are at least plausible.

Some must support their parents; in other cases the husband has been out of employment, or sick, so much that the wife has had to stay close to the payroll. A tuberculous husband is mentioned in several instances. Others have been hit by the depression. Mrs. *H.* married a ready-made family, and didn't feel that she dared add one of her own to those her husband already had. Mrs. *I.* faces the same situation except that her husband is also paying alimony to

a previous wife. Mrs. *J.* is slaving to support an imbecile sister, and sacrifices her own possibility of motherhood to the care of this unfortunate.

4. "Health" may be a rationalization occasionally, but in many of these cases it is a grim reality. Even in this educated stratum of society, a number of the childless marriages give every evidence of being due to syphilis; others to tuberculosis, still others to glandular deficiency.

5 "Dislike of children," which includes "fear of childbearing," is to me the most interesting of all the groups, for the light it throws on the attitudes and educational background of supposedly intelligent people. Occasionally the fear of childbearing is to some extent justified, as with the woman whose pelvis is too narrow for normal delivery. Other women would be perfectly willing to take the risk of a Caesarean operation; but if a woman does not want to take this risk, she should not be compelled to. Most of the cases are purely neurotic, however. Was it Oswald Spengler who said that "The modern woman has neuroses instead of babies"? This little study shows that it is equally true of the modern man!

Mrs. *K.'s* mother died in childbirth; her sister also died giving birth to her first baby. With these examples ever in mind, Mrs. *K.* will not take the risk. Mr. *L.*, on the other hand, lost his first wife in childbirth; he will not allow his second wife to jeopardize herself. Mr. *M.* did not even have that excuse; but "he adored his wife and did not want her to suffer or be disfigured."

Such cases, in which there is a fear of childbearing, shade insensibly into the cases of mere dislike of offspring. The latter cases shade insensibly into group 1, made up of persons who are so self-centered and infantile that they do not want to have to divide the spotlight with an offspring. I have tried to differentiate categories 1 and 5 by putting in the former those who did not dislike children, but liked themselves better; in the latter those who, while doubtless not disliking themselves, had an active dislike or antagonism toward offspring.

Mrs. *N.* likes children but her husband is terribly annoyed by them—to the extent that they have gradually lost all their friends who have children. Mr. *O.* is like a number of others who were unhappy in childhood, and will not bring children into the world to go through such an ordeal. Mrs. *P.* "can't stand to have a child around"; Mrs. *Q.* says children make her nervous; Mrs. *S.* had a particular aversion to children, and died after an abortion. Mrs. *T.* is dominated by a neurotic mother, who doesn't like children (she ought to know). Mr. *U.* says it is hard enough to get along with a wife; he wouldn't want to undertake responsibility for a child as well. Mrs. *V.* has seen so many unhappy marriages where there are children that she doesn't want to add to the number. Mr. *W.*, who takes life seriously and children more so, was so upset by the news of his wife's pregnancy that he became violently ill and she consented to an abortion to save his life, so to speak. And so the story continues, — a striking commentary on present-day education.

6. Eugenics is, as the reader will note, invoked by only one couple in every twenty, among the voluntarily childless. In other words, the part that eugenic motivation plays in producing childlessness is virtually negligible. The cases in which it is invoked seem to be legitimate enough, mostly involving the existence of mental disease in the ancestry.

7. Finally, there are a few cases in which husband and wife are so inharmonious that they do not want children, perhaps thinking that the presence of offspring would complicate matters still further and also create difficulties in case of a divorce. Several of these are mixed marriages between a Protestant and a Catholic, in which the difference of religion is invoked as an explanation of childlessness, — though one would suppose that this explanation could hardly be made by a Catholic in good standing with his church!

It will be remembered, in conclusion, that the causes here cited as leading to voluntary childlessness are not given by the couple themselves. As a fact, *their* explanations might be altogether different. This study has been made by asking the close friends and relatives of childless couples to tell what they, and not the couples themselves, think is the real motive for the childlessness. I suspect that in this way a more objective and accurate picture is obtained than if the husband and wife told their own story. At least, both methods of inquiry should be followed. The present method indicates that the great bulk of the voluntary childless marriages are motivated by individualism, competitive consumption economically, and an infantile, self-indulgent, frequently neurotic attitude toward life. It seems to me, therefore, to suggest several lines along which the education of young people might profitably be changed in the interest of society.

✦

(*Continued from page* 464)

Time. Genetic experimentation in cancer must necessarily depend on the breeding of relatively pure stock before valid conclusions can be reached. Pure stock means inbred strains, highly uniform in cancer incidence. Attempts to reach final conclusions without suitable experimental material prompted Hogben's plaintive observation that "many a sloppy piece of biological research is concealed behind a facade of flawless algebra." When we consider that Miss Slye has spent a quarter of a century in mouse cancer research, and when it appears from this statement that she has not been able to maintain strains of known cancer incidence, the impossibility of reaching valid conclusions with this material is tragically obvious. This situation cannot alter the fact that all "fair minded men" willingly concede that Miss Slye did invaluable pioneering in showing years ago that a genetic basis did exist for cancer development in mice.

The principle which Jaffe apparently implies, that there is a sex distinction in scientific research, is a very dangerous principle indeed. The objective search for truth is a difficult enough matter under any circumstances. We can hardly afford to be handicapped by giving recognition and medals to the work of a woman *as a woman*, when a man's work of similar quality would unquestionably not be accorded such recognition. The reviewer is very doubtful whether female scientific research workers will thank Mr. Jaffe for advancing this not too complimentary view.

To judge by the first sentence of his prepenultimate paragraph, it seems that Mr. Jaffe and the reviewer are at one on the main point. We agree that research in cancer genetics is just in its earliest beginnings. We are both convinced, I imagine, that a large number of different genetic factors are involved—factors regarding which very little is yet known. We both agree that Miss Slye's verses are excellent—only the reviewer does not think they have much to do with the strictly scientific aspects of 1935 cancer genetics.—Robert Cook.

In answering my first point, Mr. Cook questions whether Dr. Conklin "read all of my book with a critical eye." That is a matter which the President of the American Association for the Advancement of Science can answer best.

The second point, for some unexplained reason, has been completely ignored. Regarding my last point, we agree that conclusions drawn from most genetic experiments must be examined very carefully because of the many difficulties involved. Here there can be differences of opinion and honest ones, but, as Voltaire wrote, "we may have preferences but no conclusions."

Finally to say that I have raised "a sex distinction in scientific research" is to come perilously near being ludicrous. Rather has nature created an *emotional* distinction which stamps the scientist who cannot fairly appraise the work of those who differ with him.

For the many kind things that Mr. Cook has written about *Outposts of Science* I am, of course, very thankful. I feel, however, that The Journal of Heredity still owes Maud Slye an apology.—Bernard Jaffe.

Several names of ignored researchers were mentioned in the original review, but the reviewer questions the taste and wisdom of contemporary placing on pedestals.

In the face of facts intelligent people do not differ. We all agree what happens when one steps off the coping of a building, and there is no more reason to dispute whether a dominant gene or a recessive is easier to eliminate than there is to maintain that the circumference of a circle is four times the diameter. The genetic statements cited from the *Outposts* remain *wrong*.—R. C.

STUDIES IN THE GENUS FRAXINUS

II. Data on the Flowering and Fruiting Habits of Three American Species of Ash—of Possible Economic Importance in the Production of Rapid-Growing Forest Trees

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THE genus *Fraxinus*, including several species of outstanding economic importance, is of particular interest to the tree breeder since several of the species are dioecious. It may therefore ultimately be possible to produce first generation hybrid seed cheaply and in quantity by establishing the desired parental species in isolated plots and removing the male trees of one species. Such an objective will require the compilation of data on the flowering and fruiting habits of *Fraxinus*. The following paper reports an initial attempt to gather such information for three American species. The work was carried out at the Arnold Arboretum of Harvard University in the spring of 1934 under the auspices of the Harvard Forest. The authors are indebted to the late Director of the Forest, Dr. R. T. Fisher, and to the other members of the Forest for advice and encouragement.

Crossability: The genera of the flowering plants differ in the ease with which species crosses can be made. *Fraxinus* is apparently fairly favorable material; a number of crosses are on record.[1] The clearest case is that of the hybrids produced at the Royal Botanical Gardens at Kew as the result of pioneer work by Dr. Augustine Henry. These hybrids (*Fraxinus excelsior* × *F. lanceolata* and *F. excelsior* × *F. oregona*) were seen by the senior author in 1934. They were at that time thrifty young trees, the cross *F. excelsior* × *F. oregona* being particularly vigorous.

Sex

Fraxinus americana the White Ash, *F. pennsylvanica* the Red Ash, and *F. pennsylvanica* var. *lanceolata* the Green Ash were strictly dioecious. Two hundred trees of *F. americana* were examined in the neighborhood of Boston, Mass. Of these 67 per cent were found to be females and 33 per cent were males. The males can be distinguished in the early spring by the larger flower buds which begin to swell conspicuously while the female buds are still apparently dormant. *Fraxinus quadrangulata* the Blue Ash, as represented in the living collections of the Arnold Arboretum by trees from Ohio and from Missouri, is monoecious.

Males: In *F. americana* and *F. pennsylvanica* male trees were found to blossom irregularly. They may bloom several years in succession and then go one or more seasons without blooming. In 1934 the pollen of *F. americana* was shed from May 1st to May 6th, individual trees shedding pollen for about four days. The fertility of the pollen in *F. americana, F. pennsylvanica,* and *F. quadrangulata* varied from 92 to 99 per cent. Except in the case of *F. quadrangulata* pollen can be forced successfully by bringing twigs into the greenhouse. Beginning in the middle of April, it was possible to force pollen in nine days; two days before the trees came into bloom out of doors the time had been shortened to less than a day. Pollen stored for three weeks seemed normal under the microscope and caused fertilization when applied to stigmas of *Fraxinus*. Pollen stored for ten months contained a high percentage of apparently normal pollen grains and germinated readily on artificial media. The pollen yield was conspicuously lower in

Fraxinus quadrangulata, barely one-third as much per flower cluster as in the other species.

Pollen color varies considerable in the various species and seemed to be constant within each species. Pollen collected in quantity and examined in glass vials gave the following values with Ridgway's color chart: *Fraxinus americana,* Primuline yellow; *F. pennsylvanica* var. *lanceolata,* Mustard yellow to Amber yellow; *F. quadrangulata,* Barium yellow.

Females: Fruiting years are irregular. The same tree may fruit several years in succession and not all the Ashes of any one region fruit in the same year, although there are certain years when the seeding is much more general than in others. Female flowers are not easily forced in the greenhouse. They may be forced on the tree by covering the twigs with cellophane bags in the early spring. Once the flowers have emerged from the bud the twigs may be removed to the greenhouse and used for pollination studies. In 1934 the stigmas were receptive for a period of four to six days.

Conclusions

Quantity production of hybrid ash seed does not seem impossible. It is favored by the strict dioecism of some of the economically most important species, by the good pollen, and by the pollen viability. Short blooming periods present serious but apparently not unsurmountable obstacles.

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A NEW BUREAU OF HUMAN HEREDITY

British Eugenists Organize to Collect Human Genetic Material

THE object of this Bureau is collection on as wide a scale as possible of material dealing with human Genetics. Later, the tasks of analysis of material and distribution of the information available will be added.

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Material should be given with all available details in regard to source, diagnostic symptoms and the name and address of the person or persons who vouch for accuracy. All such details will be regarded as strictly confidential.

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A DEFENSE OF BASIC PLANT PATENTS

From the Plant Breeder's Point of View

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SINCE the passage of the Plant Patent Act in 1930, and particularly during the last few months, considerable uncertainty has arisen concerning its interpretation. This uncertainty of the law which is characteristic of so many acts of Congress, until clarified by the courts, has promoted much discussion concerning numerous points of issue. Some of the views held by geneticists and Plant Breeders have been published from time to time in the JOURNAL OF HEREDITY. Until the courts define this Act in one or more test cases any discussion as to which plants should be granted patents and what protection a patent should provide is in a way a waste of time. Yet such discussions may be worth while if they aid in bringing about a clearer understanding between plant breeders, patent attorneys, the Patent Office, and the courts.

The chief point of issue seems to be whether Congress intended plant patents to be granted to the asexual progeny of one plant, in genetic terms a clonal line, or to a group of plants which approach the claim description with a certain degree of closeness. Interest in this uncertainty of the law was greatly increased when Plant Patent No. 141 was granted for a super-double nasturtium. This has been called a basic plant patent by Eyster and Burpee,[4] because as the claim reads it is intended to include any nasturtium whose flower form is covered by the description, regardless of color, habit of growth, or other plant characters.

Cook[2] has questioned the authority of the Patent Office in granting this so-called basic patent. He seems to be of the opinion, as are a number of geneticists with whom the writer has discussed the matter, that unless a patent is granted to and protects one specific clonal line the law will be too difficult to enforce. A number of questions regarding this controversial subject have been raised by those who hold that plant patents should be granted on a broader basis. The latter group wonder if such broad patents would be more difficult to enforce than ordinary mechanical patents. In cases of litigation would the courts have more difficulty in determining the bounds of protection for a broad plant patent than for a mechanical patent which protects a principle of design? Might it not be just as difficult for the courts to decide whether plants allegedly being sold in infringement were of the same clonal line as the patented variety or a slight variation of a very similar but not identical genotype? The writer is of the opinion that if the courts adhere to the "one clonal line" interpretation, plant patents will be rendered practically worthless in many cases. Under such circumstances a slight variation from a patented variety could be marketed without danger of an infringement suit. This variant might differ from the patented variety in some minor character of no economic importance, yet it would be a different clonal line.

It is not improbable that clones similar but not identical to varieties already patented will arise from time to time. By careful breeding such similarities could undoubtedly be produced in spite of the high degree of heterozygousity and uncertain genotype of most horticultural varieties.

The Plant Patent Act was intended by Congress to provide economic encouragement to practical plant breeders. The opinion was expressed in Congress at the time the Act was up for discussion that to make patent protection available to asexually propagated plants would not only aid that branch of American

agriculture which produces and sells such plants but would greatly encourage the breeding and introduction of improved varieties. Thus the public at large would benefit. The basic idea of the Constitutional Convention in making provisions for patent laws was to promote progress for the public good rather than for private advantage. The possibility of financial reward is the one logical incentive for such progress. By making the protection granted a plant too narrow the very purpose of Congress will be defeated. Narrow protection will eventualy result in certain patents becoming worthless. The encouragement for inventive progress will diminish with each case of this nature until the Plant Patent Law will have little effect in promoting the breeding of new and useful varieties. No one seems to know just how Congress intended the technicalities of the law to be interpreted. Possibly that body was not aware of all the biological and economic implications of tne Act, yet we do know what Congress intended it should accomplish.

Unfortunately, the first and thus far the only court decision involving a plant patent was based on a "one clonal line" interpretation of the law. This was the decision handed down by the United States District Court for Eastern Ohio invalidating Plant Patent 110. The case has been discussed by Bebb[1] and more recently by Cook.[3] Plant Patent 110 was granted to M. H. Horvath in 1934 for an erect growing form of *Berberis thunbergia.* The plaintiff in the case charged the defendant with infringement. The chief basis of the court decision invalidating the patent was evidence that the plant in question was introduced to the public prior to passage of the Act in 1930. One part of the decision, however, raises the question as to whether plants being sold by the defendant might not have had separate origin. The following is taken from the text of the decision: "From a consideration of all of the evidence upon the subject, I feel unable to say that it would be impossible to reproduce or duplicate substantially the character of plant of the plaintiff without cuttings from the Horvath plants. Conceding that the plants of the plaintiff and of the defendants have similar characteristics, the proof is not clear and convincing that the plaintiff must have appropriated plants or cuttings belonging to Horvath or his assignee."

If the decision of this court is taken as precedent, we are led to believe that proof of independent origin alone would furnish protection from infringement suits regardless of the similarity of the two plants involved. This would not only allow sale of very similar varieties but identical varieties as well if it could be proven that they were not reproduced asexually from the patented plant. Independent invention is of no defense in a suit involving an ordinary patent. In this one respect at least any plant patent act must follow the principles of ordinary patent law if it is to be of any value whatsoever to horticulture and to the public.

As this decision was rendered by a minor court, it may or may not have precedent in later court proceedings. Nevertheless, it is of special significance that this first court decision heads in the direction of a useless Plant Patent Law.

Plant Patent No. 141, discussed by Eyster and Burpee,[4] presents a very unusual case. A super-double nasturtium which is female sterile but possesses fertile pollen was discovered by the patentee. This was found to differ from the ordinary double nasturtium by one dominant gene. It was, therefore, very simple by crosses and back-crosses with ordinary double nasturtiums to produce numerous colors and other variations all possessing this peculiar type of flower. The writer* has observed at least twelve

*Until June of 1936 the writer was employed as a plant breeder by the W. Atlee Burpee Company. While in the services of that firm, he did considerable breeding work with double and super-double nasturtiums. His opinions expressed here are in no way intended to reflect the opinions of his former employers.

distinct flower colors, three chlorophyl intensities, three leaf shapes, and four habits of growth in the garden nasturtium. Any combination of these characters combined with the super-double flower type could be produced by making the proper crosses. Therefore, no less than four hundred and thirty-two distinct clonal lines of super-double nasturtiums are within the realm of possibility. Had the Patent Office refused to grant this so-called basic patent, the patentee would have been required to take out four hundred and thirty-two separate patents in order to fully protect his invention. This might have been good business for the United States Patent Office but mighty poor business for a commercial plantsman.

Another case similar to the super-double nasturtium, namely, one in which a character dependent on one gene which can be readily transmitted through the pollen of an otherwise sterile plant, may not arise again for years. This particular case is perhaps not important in itself as the super-double nasturtium is a mere oddity bound for the horticultural graveyard after a season or two of prominence as the result of extensive publicity. Its introduction has probably been worth while, however, if it aids in clearing up the question of broad plant patents vs. patents on a single clonal line.

When a breeder discovers a new plant character of economic importance, his discovery is comparable in a way to a new mechanical design. When he combines that character in one plant with other characters which render the resulting variety salable, his act can be compared to the application of the new design principle to a commercial product. In either case there is something basically different between the new and the old. The mechanical inventor is given protection broad enough to cover his principle of design. If plant patents are granted to one specific clonal line, the plant breeder cannot be assured of any lasting patent protection.

For example Plant Patent No. 4 was granted for a thornless dewberry. The breeder of this variety was the first to discover a dewberry possessing this important economic character. By hybridization it would undoubtedly be possible to produce another thornless dewberry variety, similar to the original, just as good for commercial purposes, but different enough that no one could doubt its being a distinct clonal line. Should this new thornless variety be granted a patent, and should it be allowed on the market in free competition with the dewberry supposedly protected by Plant Patent No. 4? Under such circumstances actual protection would not exist. It may be argued that the breeding of still better strains of thornless dewberries would be impeded if Plant Patent No. 4 was considered a basic patent on the thornless character. Indeed it would be an imposition on the public if this patent was interpreted on a basis so broad that no new thornless dewberry could be sold regardless of its other qualities. A new dewberry possessing superior quality or any other desirable new characteristics together with the thornless character should be patenable, but not unless it is distinct from the original in some character of horticultural value.

Regardless of what Congress may have intended and regardless of what views are held by the Patent Office and attorneys, plant patents granted on a "one clonal line" basis may have little value to the breeder. It is suggested that patents on varieties which are of importance because they represent a new combination of characters should be broad enough to protect them from infringement by essentially similar clones not of distinct horticultural value. It is further suggested that patents on plants which possess some new character not before known to horticulture should be basic enough to protect them from infringement by varieties which possess that new character but differ in some other respect unless these further differences render the clone of distinct economic importance in themselves.

As Cook[3] has suggested, the law needs immediate revision before later court decisions render other plant patents invalid

and discredit the whole idea of patent protection for plants. Such a revision must come about if the Plant Patent Act is to be of lasting value to American horticulture. Every effort should be made on the part of horticulturists to encourage the introduction and passage of a revised law. In the writer's opinion every effort should be made to encourage a revised law broad enough to assure protection from infringement by very similar clones.

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Editor's Note. Mr. Barron's discussion is of great interest as presenting the view of a practicing plant breeder regarding the Plant Patent Law. One cannot avoid the feeling however, that Mr. Barron's may much underrate the value of a "one clonal line" interpretation of the law. In actual practice a great many varieties have a rather short period of popularity—before they are headed for the "horticultural grave yard." Thus in actual fact a "clonal patent" would in most cases give excellent protection for several years, during which the "economic cream" of a great majority of new varieties would be skimmed. Especially is this true if the name of the variety and the new form were linked together by some form of registration. This set-up would give the originator or his assignee no little protection through injunction against infringers. This should be a very formidable deterrent to "pirating" a new variety.

In actual practice it is possible that protection under a "clonal" law might be better than protection under a broader law. We must bear in mind that botanists have not been completely successful in evolving accurate verbal diagnosis of species differences. Since this botanical experiment in plant description has been going on with varying success since Linnaeus' time, it may be that a valid definition of varieties differing only in a few rather variable characters may be virtually impossible. As a matter of practice it might therefore prove impossible to make plant patent claims "stick" unless reference is to a definite clonal variety or to a definite type plant.

There is a very real difference between trying to differentiate plants by descriptions and drawings, and in trying to differentiate mechanical devices by that means. While mechanical devices are undoubtedly simpler in their patentable elements than plants are, nevertheless the verbal complexities and perplexities which are built up to circumscribe new developments in mechanical patent claims are astonishing, as anyone can learn by reading a few patents. The difficulty of a complete, unequivocal description of a plant on such a basis appears to be very formidable indeed, and may well in practice be unworkable.

The relative simplicity of a registration system in which a new form and a name are linked together seems to have practical advantages which should not be overlooked in working toward a revision of the Plant Patent Law.

Testing of Patented Plants

November 9, 1936.

To the Editor:

With reference to your article on Plant Patent 110, it does seem as if it ought to be possible to get a group together to plan revision of the law along the lines you suggest.

It is hardly to be expected that the people responsible for the law and its administration will be of much help—for it will first be necessary for them to admit serious errors.

It is too bad that such a large percentage of patentees should have paid out good money and received patents obviously invalid. Some of them could probably be re-issued. There is one basis of invalidity that is not much stressed, i.e. in public use or on sale for more than two years prior to the application for patent. This is not the same as "introduced to the public prior to the approval of the act."

The Burbank plants, I understand, were actually *sold* many years before filing the applications, which, if true, would have prevented issue of patents.

Mere "testing" in various soils and climates seems proper—with no time limit unless there is "public use" or "sale" more than two years before filing the application for patent or "introduction to the public" before approval of the Act, whatever that may mean.

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FROM SORANOS TO GOODYEAR

The Story of Man's Long Struggle to Control Human Fertility by Means Less Brutal than such Malthusian Checks as War, Starvation and Disease

THE human race has been struggling for thousands of years to develop a safe, sane, and reliable means of peacefully controlling population growth, yet during modern times there has been available no comprehensive medical history of contraception. When Professor Norman E. Himes, of Colgate University, undertook some ten years ago to remedy this situation he soon found that in endeavoring to map historically that virtually unknown land he had undertaken a real job of exploration. His trek soon brought him smack up against an intangible but none the less real obstacle of ominous proportions, that secret, sacred, and awful psychological cliff which the African savages call "juju."

As late in the history of the world as the year 1925 A. D. the phrase "birth control" struck fear in the hearts of many modern medicine men, and few were ready to accompany Professor Himes back along the trail which he had undertaken to follow to its beginnings long before the golden ages of Greece, Rome and Egypt. Letters to authorities suspected of having important knowledge about this none-too-well marked trail brought little response and less information about the great tabooed plateau which our pioneering scientific Tarzan had assayed to explore. Not even world famous libraries had scientifically catalogued in their most secret chambers the trails leading to the mysterious realm, and before Professor Himes could go far along his journey he was forced to study even the science of philology to make available original sources.

At length the Social Science Research Council came to Professor Himes' aid. Armed with a grant from that learned scientific organization he travelled far enough to make a comprehensive survey of the birth control movement in England. This progress was noted by the National Committee on Maternal Health, which was scouting in the territory to which Professor Himes wished to journey. The present volume* was issued under the supervision of this medical committee and the book was reviewed before publication by a highly competent medical editorial board.

If the above introduction to a book of serious scientific nature seems needlessly dramatic or heroic, one need travel even today but a short distance along the pioneer path on which Professor Himes has gone so far, to appreciate the herculean nature of his labors. A glance at the Federal laws on the subject of contraception might also assist the reader to get into the proper atmosphere to appreciate the tremendous labor involved in his book. It had hardly issued from the press when the American Medical Association issued a report of a year's study which is quite a contrast to the scientific work of Professor Himes. When we realize the powerful taboo that is attached to the subjects of sex and the reproduction of the race, we may possibly feel considerable sympathy with the position of the practicing physician, and understand that the task of the strictly scientific worker in this field is not an easy one. There is an abundance of general and special medical history in circulation, but this one aspect, a most important one to human welfare, appears to have been almost totally neglected.

In his foreword Robert L. Dickinson, M. D., secretary of the National Committee on Maternal Health, says of this book: "Its broad range is in no wise restricted to the word 'medical' in the title, because social and economic implications

*HIMES, NORMAN E. *Medical History of Contraception.* Medical Aspects of Human Fertility Series issued by the National Committee on Maternal Health, Inc. Pp. xxxi+521. $7.00. Baltimore: Williams & Wilkins Co. 1936.

can no more be disregarded within this area than they can be in the diagnosis and treatment of disease or in any other section of preventive medicine." The *Medical History* Professor Himes projects as the first of three volumes on the general subject of birth control. The other aspects which he hopes to discuss later are: (1) the social and economic development of birth control, and (2) the relation of birth control to certain fundamental aspects of modern population theory.

As might have been expected, Professor Himes found many curious practices among savage tribes which were supposed to prevent conception, but perhaps none of these were more magical than some of the charms and "old wives tales" which are passed over the back fence today. A rather common belief among primitive tribes as well as among modern peoples, is that certain herbs or medicines when taken by mouth will prevent conception for longer or shorter periods or will induce abortion. Dr. Himes' conclusion may therefore come as a surprise even to many well informed biologists, that "no drug has yet been discovered which, when taken by the mouth, will induce temporary sterility . . . prevent conception or abort."

It has often been said that birth control is nothing new and that it is as old as sinful man himself. This may have led many to wonder if the savage tribes had any knowledge of contraceptive birth control. On this point Professor Himes says: "A mere enumeration . . . of the population checks operating among contemporary primitive tribes and, therefore, by inference, among our more distant ancestors, suggests, and correctly, that conception control played but a small part in limiting the growth of numbers. It seems clearly established that infanticide and abortion were more frequently practiced than conception control."

Like some of us moderns, some primitives also had their periods when intercourse was taboo. But they also had tabooed days for births. For example, in Madagascar, "All children born on certain unlucky days were put to death to prevent them from bringing bad luck to their families." It may be readily seen that if enough unlucky days occurred in the course of a year the population could easily be kept in hand. This method of population control may seem barbarous to civilized man, as indeed it was, but Himes reminds us that "in most uncivilized groups life is not held sacred until the child has gone through a formal ceremony or tribal recognition, usually marriage."

It is not, however, correct to say, according to Himes, that preliterates had no knowledge at all of preventive methods of birth control. For example, by trial and error, they sometimes hit upon a crude type of vaginal suppository. In Java an abdominal massage to tip the womb was sometimes employed to prevent conception. And in other localities puncture of the male urethra at the base was a ceremonial practice in early manhood. These rather brutal customs lessened to some extent the chances of conception. Sexual irregularities, not completely unknown to modern man, were practiced among our more distant ancestors to keep down the birth rate.

The fact that intercourse among the young people was promiscuous in British New Guinea without showing an unduly high number of conceptions has led travellers to wonder if the inhabitants of that country did not possess knowledge of some reliable contraceptive. "The infertile promiscuity," says Himes, "is probably to be explained on Hartman's theory that girls have an infertile period for three or four years after the onset of menstruation. They have no children because they are biologically immature, not because of any diffused contraceptive technique. Puberty and sexual maturity are not identical as commonly supposed."

While the primitive races knew little about contraceptive birth control as we know it today, Himes says: "The desire for control is neither time nor space bound. It is a universal characteristic of social life. * * * It is abundantly clear that many primitive peoples thought in Malthusian terms long before Malthus.

The women of preliterate society knew all about Malthus' main point; that too rapid increase endangers support. Malthus' Essay was essentially a learned inductive proof of an obvious thesis long understood and long acted upon."

Coming forward in history to the Egyptians, Himes says that while contraceptive techniques were recognized in Egyptian medicine as early as 1850 B. C. they were not diffused nor democratized. In fact, he says, it is one of the major generalizations of his book that contraceptive knowledge, at least of a quasi-reliable, harmless sort, did not begin to become a mass phenomenon until late modern times (early nineteenth century). And, he says, the incomplete process is still continuing.

The early Jews had a smattering knowledge of contraception as is shown by the fact that the moistened sponge method was known to the authors of the Talmud. The conditions under which this method might be legitimately used were also laid down, says Himes. " 'The Talmud-Rabbinic law does not consider the use of contraceptives, as such, immoral or against the law. It does not forbid birth control, but it does forbid birth suppression'." Himes thus quotes a learned Hebrew authority. At all events, says Himes, it is clear that the ancient Hebrews manifested a desire to avoid both excessive sterility and excessive fertility. This he found to be a characteristic of every civilization he studied, — another major generalization of his book.

Among the ancient Greeks was found not only a rational population policy in Plato, Aristotle, and others, but also a physician, Soranos (98-138 A. D.), whose "conception of contraception" was the greatest until the nineteenth century. The first mention of vinegar as a contraceptive was mentioned by the physician Aetios, who owed much of his knowledge on this subject to Soranos. He says that the ancient physicians who treated the subject of contraception were of the greatest of all time, men who have a permanent place in the history of medicine. It was these great physicians "who thought contraception worthy of discussion and analysis; who held it to be a legitimate part of medical practice; who left for the modern small-fry the idea that it was obscene."

The Dark Ages after the fall of Rome were especially dark for contraception. Himes says that the Middle Ages perhaps did not know as much about contraception as did the Egyptians, Greeks, and Hebrews. After tracing medical contraceptive literature in the Orient and Islamic world, Himes considers the activities of Bentham, Mill, Place, Owen, Knowlton, and Drysdale during the early part of the nineteenth century. Their formulation of the social theory of birth control, he says, is accepted by the majority of economists and sociologists. He further considers Annie Besant's trial for publishing Dr. Knowlton's book and her law of population. In his history of modern contraceptive methods Himes brings out the fact that Charles Goodyear's discovery of vulcanizing makes him perhaps not only the "greatest name in rubber" but also one of the greatest names in contraceptive birth control. The Oneida community experiment in this country is treated briefly, as is the more recent birth control movement in England, Holland, and Germany.

As has been mentioned in future volumes Himes will consider contemporary problems and personalities of birth control more in detail, but he does discuss briefly here the contributions to the movement by Drs. Havelock Ellis, Dawson of Penn, William J. Robinson, Abraham Jacobi, and William Allen Pusey, the later two physicians having made birth control the subject of their presidential address before the American Medical Association. The work of Margaret Sanger is evidently reserved for a later volume.

In a survey of various birth control clinics Himes noted that two-thirds of the women admitted to these clinics had had previous experience with contraceptive methods. He notes that the clinical method now common in many countries is about 93 per cent effective, and that most of these clinics are operating eugenically.

Himes believes that contraceptive birth control is a safe and effective method of peacefully adjusting population growth to resources and that it should rightly take the place of the more savage and barbarous methods of infanticide, abortion, war, and sexual irregularities which have hitherto been used to control the birth rate. A keen student of population himself, he takes issue in no uncertain manner with those who fear that contraceptive birth control will cause race suicide. He says:

I have never met in all history with a depopulationist (by which I mean a bewailer of declining population) nor with a population booster (one who is never satisfied with the present rate of increase) who ever understood the economic and sociological theory of demographic movements. Never do such writers really understand the phenomena they mouth about. Never do they see demographic phenomena as adjustment or equilibrium phenomena. In a word, this is because the depopulators or optimists start with preconceived notions about what the people, under the circumstances of the time, *ought* to do. The masses know more about these matters than do statesmen, physicians, and pamphleteers. The reason is briefly that it is the masses and not the advisors, who feel the pressure set up by economic and social situations.

Perhaps it is fitting to close this review with a statement from the preface of this book:

My study does not support the thesis that those cultures which employed anti-conceptional practices died out because of it.

From the universality and antiquity of anti-conception I infer that it fulfills some fundamental human need. It has shown great sticking power. It has not only survived, it has grown increasingly strong.

GUY IRVING BURCH.

✦

THE EUGENIC ROLE OF CONSUMERS' COOPERATIVES

A Suggested Plank for a Eugenics Platform

THERE has recently been considerable discussion in the pages of the JOURNAL* regarding various phases of the problem of evolving a workable eugenic program. The writer was especially interested in the discussion of what constitutes true fitness. Our standards of human worth must indeed vary according to the character of the civilization we propose to build. Rude, barbaric civilizations will require a different type from cultured and highly refined civilizations. A movement that has made rather remarkable progress in recent years, and that may have important implications in this connection, was not mentioned in any of these discussions, and the purpose of this note is to remedy this rather remarkable omission.

In judging what type of men is best fitted for an advanced civilization, it is necessary to avoid certain common pitfalls. One of these is, in my opinion, the excessive value set upon speed and efficiency in modern education, as distinguished from the less "practical" virtues, such as ability to cooperate with others and to form sound judgments on important life-issues. Recent tests suggest that there may be a rather low correlation between school-intelligence and ability to arrive at sound conclusions relative to such issues. And certain facts of history and current events would seem to confirm this conclusion. It is a tragic commonplace that kings, princes, diplomats, soldiers, and traders — whom we would probably class as eugenically superior men—have sometimes been responsible for maintaining incredibly stupid and vicious social policies, while common unskilled laboring men have on occasion

*"A New Eugenics Program," May, 1936; "The 'Market Test' of Eugenic Fitness," June, 1936; "The Geneticist—is he Nazi or Bolshevik," July, 1936, and "The Place of Environment in a Eugenics Program," August, 1936.

shown marvellous wisdom in government. Excellent examples of the breakdown of government by the "fit" are to be found in the present impasse in which European diplomacy finds itself. In contrast we may cite the very real social progress made by the Consumers' Cooperative movement and the experiments in government ownership in such countries as Sweden, Denmark, Finland, New Zealand, and Australia.

It must be obvious to every intelligent and well-informed person that in a complex civilization the ability to think clearly on such basic issues as sociology and economics is far more important than personal prowess, speed, and efficiency. These virtues, though good in themselves, will, if not carefully guided, intensify the conflicts within society and thus lead to its destruction through war, exploitation, and social injustice. It is well to move rapidly, but it is infinitely more important to move in the right direction. Nazi Germany is moving,—but there is a very great question whether the sum total of her movement is in the direction of social progress.

Emphasis upon speed and efficiency in the individual is the natural correlate of a competitive economy. In a cooperative economy emphasis would naturally tend to fall upon ability to cooperate with others. Just now civilization is suffering from an excess of speed and efficiency with no adequate power to direct the forces thus set in motion.

It is even possible that virile, full-blooded and aggressive—or at any rate domineering — types of men, with a strong development of the animal nature, would not be the best possible type for a social utopia. The ancient Romans were peerless warriors, but proved themselves much less adaptable in the important matter of wisely using leisure time. The inhabitants of a planned civilization would necessarily be of a finer grain physically and mentally if such a civilization is to be maintained. This type would be better fitted for the intellectual and cultural activities which would abound in such a civilization; men of coarser grain would tend to gravitate into sensuality the moment they were freed from struggle and hardship.

But however this may be, it is certain that superiority can be made socially effective only through education. It will be agreed that the spirit of conservatism has been a mixed blessing to mankind. Without conservatism a stable social economy is hardly possible, but entering as it does into every realm of life, and especially into the mores, it tends to retard progress just where it is most needed, while progress in the control of physical nature, and the development of implements of destruction, goes on with accelerating speed. The result is catastrophe. For these reasons an important function of education should be to combat a spirit of overconservatism with regard to social progress. Yet we find exactly the reverse in most nations of the world. Education in the average European school, for example, has been education in narrow nationalism. It is significant that the Fascist nations of Central Europe have been characterized in the past by an especially authoritarian type of education. Through the medium of schools and the printed page, the stupidities and the banalities of the older generation are imprinted upon the minds of the younger generation. And though the logical way to educate a nation is to educate the younger generation, this is made impossible by the fact that the educational policy of the schools is controlled by the older generation. Hence the only way to reach the children is first to reach their parents. But this may be extremely difficult, if the radio, the press, and the moving pictures, are controlled by selfish, predatory interests. Thus a vicious cycle is set up. And in a highly mechanized, industrial civilization, this makes for results that are not always pleasant to contemplate.

As an effective method of breaking the vicious cycle of wrong institutions and of a wrong educational system, the Consumers' Cooperative Movement appears to be far superior to any other

social adaptation yet evolved. Its marvellous effectiveness is due to the fact that it operates upon the principle of the entering wedge. Most social movements depend for their success upon some kind of political victory, and if victory is not attained, the efforts put into the movement are largely wasted and the people who sponsored it are still more impoverished. But the Consumers' Cooperative Movement grows from small beginnings and consolidates its gains as it goes. As it appeals to the peoples' immediate, as well as to their remote interests, it exerts a never-ceasing fascination over their minds; it is the one movement that so far does not seem to grow conservative with age. And, moreover, it trains its leaders as it goes, thus escaping the pitfalls of inexperience and inefficiency which beset the political revolutionist. No one can accuse it of being utopian or visionary, for it is a going concern of such proportions that it can hardly be ignored; it now controls twelve per cent of the retail trade of Great Britain and forty per cent of that of Sweden. The International Cooperative Alliance has over a hundred million members.

It is to be greatly desired that the effect of this movement upon the birth-rate be made a subject of special inquiry. Such effects ought to be very pronounced in countries where the movement is strong. We feel that such an inquiry would be an excellent undertaking for the American Eugenics Society, now that the consumers' cooperatives are beginning to spread so rapidly in this country. The recent marked change in birth trends in Sweden, which appear to have wiped out the adverse birth - differential heretofore found in all European countries, may be directly related to the growing influences of consumers' cooperatives in that country.

So far from looking upon social and economic reform as opposed to eugenics, I consider it absolutely essential for the success of the latter. It is true that such reform would have a dysgenic effect if it were allowed to end in itself. But is is inconceivable that this should take place. In a planned social order, not only will the inherent qualities of men be easier to determine, but eugenic legislation will be far easier to secure than in an order based upon cut-throat competition, and the facilities for eugenic education will be far more abundant than today.

It seems to me that the Cooperative Movement offers special opportunity for experimentation in the field of developing cooperative colonies along eugenic lines. If colleges for married couples could be devised, along the lines of the Danish Folk Schools, the dysgenic influence of present-day colleges could perhaps be overcome in a very great degree. Cooperative nurseries, playgrounds, libraries, and educational centers are a common feature of the big cooperatives of Europe. Such institutions, if built along eugenic lines, would help to promote both the Cooperative Movement and the cause of race-betterment. But perhaps the greatest contribution of the Movement to eugenics will continue to be its levelling down of social and caste distinctions. By altering the economic and social outlook of the upper classes and raising the living standards of the lower classes, it will help to abolish the differential birth-rate. And by bringing economic processes under democratic control, it will greatly facilitate eugenic thinking regarding social problems.

The apparent increasing conviction on the part of eugenists that they cannot ignore social and economic problems is in my opinion a very encouraging development. No movement is secure in a civilization that is periodically or chronically teetering on the brink of disaster. The continued progress of the sciences is dependent upon adequate leisure and security for the investigator. Should another world war disrupt civilization, the resulting social chaos would retard, and it might completely halt, the Eugenics Movement.

It is for this reason that all who have the cause of race-betterment at heart should promote by every means at their

command sound movements for social and economic justice. And I am convinced that the Cooperatives offer by far the best solution yet devised. By bringing about a planned economy, they will furnish an invaluable breathing-spell for the social scientists, setting them free from the struggle for bread and the battle against the anti-social forces of a competitive society, to devote themselves to the more subtle problems of human betterment. It is highly probable that in a cooperative civilization the radio, the press, the moving-pictures, and other organs of publicity will be controlled by those types of men who are best fitted for the task of molding public opinion along socially desirable lines. The numerous periodicals now published by the Cooperatives are so controlled. There is every reason to believe that this will be true on a worldwide scale, if and when the Cooperative Movement becomes supreme. Hence the eugenist will find his best strategy in promoting that movement. Such collaboration is greatly needed today. Though the Cooperatives are growing, the danger is that they will not grow rapidly enough to prevent the destruction of civilization through war and a resultant return to barbarism and chaos. Mr. James Warbasse, President of the Cooperative League of America, expressed the fear that such a destruction would carry the Cooperative Movement with it. In his words, "That would indeed be a twilight of the gods." But there is cause for hope in the fact that the movement appears to be holding its own even in Fascist nations, though there is considerable difference of opinion on this point among cooperators. Reports from Germany, especially, are conflicting. But Warbasse finds the movement holding its own in practically all of the Fascist nations. See especially his "Cooperative Democracy," p. 414, 1923, and his recent tract, "The Sure Way is the Swift Way." The *People's Year Book* gives official statistics on the movement in every part of the world. The extreme hardiness of Consumers' Cooperatives is one of the characteristics which distinguishes this from other progressive movements.

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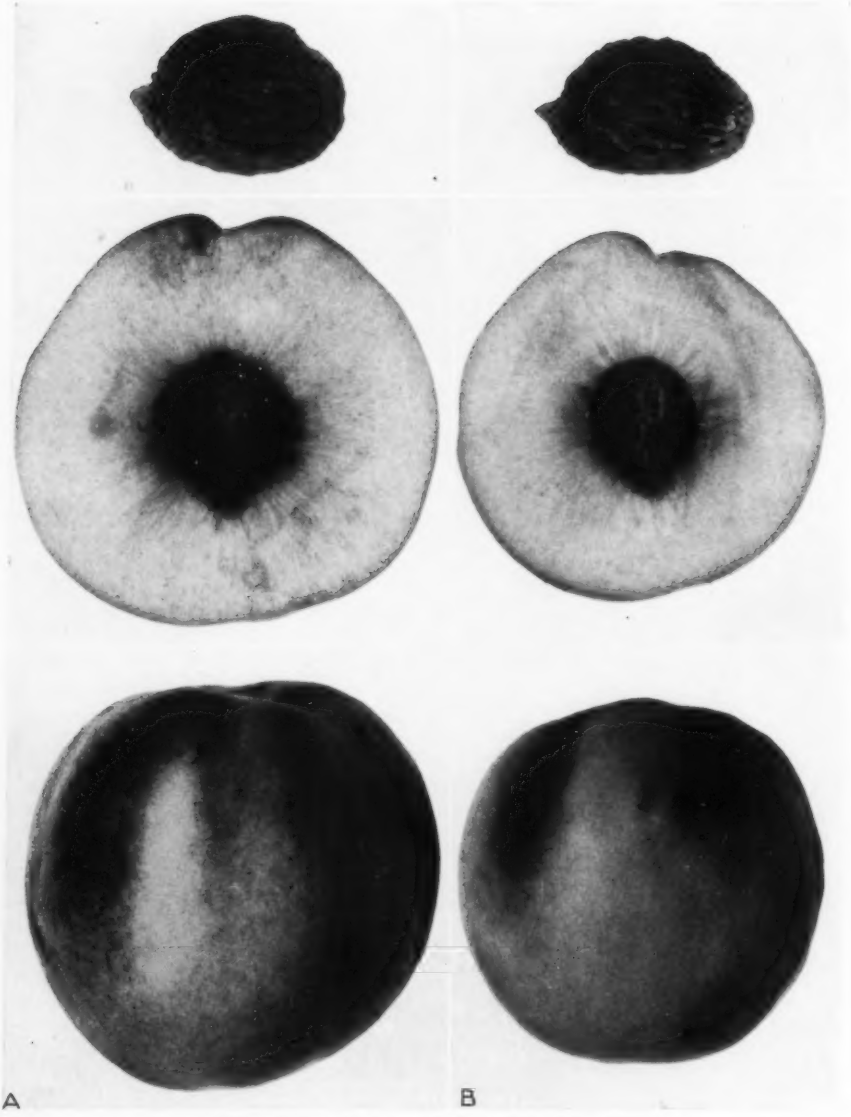
SEVENTH GENETICS CONGRESS POSTPONED

A WIRELESS dispatch to the *New York Times* on December 13th stated that the Seventh International Genetics Congress, scheduled to be held next August in Moscow has been cancelled by the Soviet Government. Apparently a group of geneticists under the leadership of C. D. Lyssenko have brought forth a somewhat "made to order" environmental genetics, better suited to Marxist idealogy than the views of "classical geneticists." It is stated that these new theories have been used to discredit such workers as Vavilov and Levit, who are accused of introducing ideas not pure Marxist. If the *Times* dispatch is accurate, this "genetic purge" has reached a point where Vavilov and Agol have been arrested for alleged Nazi leanings and Trotskiest activities.

A request for official confirmation of the cancellation of the Congress brought a denial of the accuracy of the *Times* report from Ambassador Alexander Troyanovsky, who states that Dr. Valilov has not been arrested and that the Congress has been postponed at the request of several scientists desiring greater time for preparations. An inquiry as to whether the Congress would be held a year later has not been answered at time of going to press.

A later Associated Press dispatch tells of a further genetic controversy on the occasion of the All-Union Congress of Psychiatry. Here Dr. Levit, who has recently been expelled from the Communist party, was exposed to violet attacks on account of non-Marxian racial views. It is hoped that a more connected account of Congress prospects can be given in the next issue of the JOURNAL.



A BUD MUTATION IN RATE OF MATURITY

Figure 7

Typical normal Libbee peaches (*B*) and early fruits picked from a limb variation in the same tree at the same date (*A*).

BUD MUTATIONS IN HORTICULTURAL CROPS

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DESCRIPTIONS of many striking bud mutations in deciduous and in citrus fruits have been presented by the authors in the JOURNAL OF HEREDITY and in other publications during the past few years.*

This paper presents a summary of the information which the authors have been able to compile regarding bud mutations in fruit and potato varieties. A partial bibliography of publications on this subject is appended for the use of students and other interested persons.

In many of the instances recorded in this paper the mutations have been personally observed and studied by the writers, while in the other cases reliable reports by competent observers have been accepted. It is not practicable to present as complete descriptions as might be desired, but readers interested in obtaining more complete information concerning particular cases may do so through correspondence.

There is no longer much question as to the commercial importance of bud mutations in many of the horticultural crops that are propagated vegetatively. This statement applies not only to those variations which are superior to the parent forms in one or more characteristics, but also to those which for any reason are less desirable for commercial use than the parent forms. The search for and study of bud mutations has been rather intensive during the past 20 or 25 years, and many new forms have been discovered. Some of these are now of considerable commercial importance and most of them are of scientific interest. The purpose of this article is to call attention to the progress that has been made in accumulating data regarding the occurrence of these mutations, particularly those found during recent years.

Early References to Bud Mutations

The earliest recorded fruit bud mutations that have come to the attention of the writers were reported by Peter Collinson[5] in a letter to Linnaeus April 3, 1741. He told of a russet apple produced on a green-fruited tree, and of peaches and nectarines produced on the same tree. In another letter written over twenty years later (September 17, 1765) Collinson describes another peach mutation—a nectarine growing on the same twig with a peach. Six additional instances of nectarines produced on peach trees were reported to the Horticultural Society of London by R. A. Salisbury[35] on May 3, 1808.

On April 4, 1815, Thomas Andrew Knight,[26] addressing the Horticultural Society of London, declared his belief "that many varieties of fruit which are supposed to be totally distinct have been propagated from branches of the same original tree; and that few, if any, varieties of fruit can, with strict propriety, be called permanent, when propagated by buds or grafts." He then reported a Yellow Magnum Bonum plum in his garden which had a branch bearing red fruit exactly similar to the Red Magnum Bonums. A May Duke cherry tree in his garden produced on one branch late-ripening, oblong fruit of inferior quality. He also stated that he believed the Auche pear, a certain type of the St. Germain pear, and the large Swan's Egg plum were bud mutations of similar character.

The earliest reference to a fruit varia-

*See literature references 31, 33, 36-41, and 54-62. Also see note under "Literature Cited" regarding a more complete bibliography of bud mutation.

tion in the United States which has been found is that of Cole,[7] who in 1849 reported on the Red Russet apple, which he said "seems to be a cross between the Baldwin and the Roxbury Russet." An article in his *New England Farmer* in 1850 suggested the sport origin of the variety, and another article by Sanborn in the same periodical in December, 1852, gave a detailed description of its origin as a bud sport.

Gaudichaud[19] reported to the Paris Academy of Science in 1852 an apple tree which produced several sorts of fruits, and in 1859 Oberdieck[29] described a Red Gravenstein mutant. A few years later other French and German writers referred to additional Red Gravenstein variations.

In studying the rapid increases in the number of bud mutations in fruit varieties recorded in recent years it is found that the first summary of such forms, presented by Carrière in 1856,[6] listed 35 definite foliage and fruit variants, with banana, plum, and potato mentioned as having produced "several" variations. Fawcett[17] reports that the Gros Michel banana, which is the principal variety of commerce today, was of bud variation origin. Darwin in 1868,[14] recording bud mutations that were mostly found in England, listed 46 leaf and fruit variations in horticultural fruits, 23 of which were instances of nectarines that developed on peach trees.

Cramer's list of bud mutations published in 1907[11] gives 56 instances in fruit crops and states that all the banana and pineapple varieties were of bud origin.

Powell[34] in 1898, Beach[2] in 1905 and Dorsey[15] in 1917 recorded a considerable additional number of fruit mutants, but in spite of these and the earlier records of such forms the almost universal opinion of horticulturists up to about twenty-five years ago was that fruit varieties perpetuated by asexual propagation were entirely stable and unchangeable and that new forms could arise only from seedlings.

In 1909 the senior author began studies on bud variation in citrus fruits.[36-41,59-63] These attracted immediate attention because of the large number of variants that were found. The frequency of the occurrence of these forms led to a widespread belief that the citrus fruits must be especially subject to bud variation and different in this respect from the common deciduous fruits.

However, the announcement in 1922 of the high price paid for the red mutation of the Delicious apple,[1] introduced as the Starking, gave a strong incentive to fruit growers and others to search for variations from which they might also secure a rich reward. In the next few years many interesting and valuable fruit variations were reported, mostly in apples,[9,16,21,49,53] cherries,[16,18,21,24] peaches,[16,23,25,58,68] and pears,[16,57] with fewer numbers of apricots,[69] berries,[12,13] dates,[28,48] figs,[10] grapes,[22,30,64] pineapples,[47] plums[16,51] and prunes.[46,56]

J. T. Bregger,[5] who has been particularly active in the collection of data relative to apple bud sports, recorded 125 red bud mutations in apples in 1930. A list of apple mutations prepared August 15, 1931, by Shamel and Pomeroy showed 143 red variants and 30 variations in other characters. A similar list prepared November 1, 1932, by Shamel, Pomeroy and Bregger[53] presented 221 red variants and 100 other forms. In Table I, 254 red apple variants are recorded as of May 25, 1936, with 137 additional apple variations.

The first plant patent was issued August 18, 1931. From that date to May 12, 1936, 177 plant patents were issued. Fifty-two of these covered new fruits and of this number 17, or 32.7 per cent, were of bud origin.[67]

The number of bud mutations that have been recorded in apples, pears, peaches, plums, cherries and grapes is summarized in Table I, by the character of their fruit and foliage variations. Similarly, bud mutations in the Washington Navel and Valencia oranges, in lemons, grapefruit and other citrus varieties are presented in Table II. Bud mutations in many other fruits

that have received less study from this standpoint, and in potatoes, are given in Table III.

These tables include 391 mutations in the apple, 93 in the pear, 146 in the peach, 26 in the plum, 120 in the cherry, 87 in the grape, 1,664 in citrus fruits, 124 in other fruits, and 110 in potatoes, a grand total of 2,761.

Discussion of the Data

In the apple varieties the red bud mutations are the most conspicuous, by reason of their distinguishing characteristic—a distinct color which everyone excepting those who are color blind can easily see. For this reason, perhaps, the red sports have received more attention than others up to the present time. To show how frequently such bud sports have been discovered by growers during recent years it may be of some interest to note that there are records of red strains of bud origin in 39 com-

TABLE I. Summary of bud mutations in certain tree fruits as recorded prior to May 25, 1936.

Variant forms	Apple	Pear	Peach	Plum	Cherry	Grape
Fruit Variations						
More color	254	2	3	6	6	1
Less color	21	2	10	8	—	6
Striped fruit	5	47	—	—	—	2
Russeted fruit	26	17	—	—	—	1
Larger fruit	30	13	1	3	4	45
Smaller fruit	2	—	—	—	—	11
Flattened fruit	6	1	—	—	—	—
Oblong or long	5	1	—	—	—	—
Other abnormal shapes	20	3	1	1	4	—
Early maturity	3	3	41	2	3	4
Late maturity	3	1	19	2	38	1
Early bearing	3	—	—	—	—	—
Unproductive	—	—	3	—	48	1
Seedless	2	—	—	—	—	2
Nectarines	—	—	27	—	—	—
Other abnormal fruit	8	—	—	—	—	4
Foliage Variations						
Variegated	1	2	9	1	6	4
Willow leaf	—	—	5	—	1	—
Cut leaf	—	—	2	—	1	3
Other abnormal foliage	2	1	18	1	—	1
Flower variants	—	—	2	—	7	—
Other misc. variants	—	—	5	2	2	1
Totals	391	93	146	26	120	87
Grand total						863

TABLE II. Summary of the most important types of bud mutations observed in citrus fruits. Several of the numbers given are conservative estimates, as no detailed records have been kept of the many variants observed.

Variant forms	Washington navel orange	Valencia orange	Lemon	Grapefruit	Other citrus varieties
Fruit variations					
Australian types	150	—	—	—	—
Unproductive	10	25	50	5	2
Early	2			1	41
Corrugated	200	500	100	10	17
Pear-shaped	10	5	25	—	—
Long	20	50	—	—	—
Flattened	5	20	—	—	5
Striped	5	10	5	—	—

TABLE II (Continued)

Variant forms	Washington navel orange	Valencia orange	Lemon	Grapefruit	Other citrus varieties
Brown-Spotted	2	—	—	—	—
Yellow	30	5	—	—	3
Reddish-Orange	2	—	—	—	1
Dual	1	1	—	—	—
Coarse	25	40	5	—	—
Smooth	10	10	—	—	1
No navel	25	—	—	—	—
Persistent-style	—	2	—	—	—
Small	10	50	—	—	—
Seedless	—	1	—	1	2
Pink flesh	—	—	1	15	—
Orange flesh	—	—	1	—	—
Dry flesh	10	1	—	—	1
Miscellaneous	10	15	10	—	5
Foliage variations					
Variegated	20	40	5	—	5
Willow-Leaf	2	10	—	—	10
Misshapen-Leaf	—	1	—	—	—
Rolled-Leaf	2	—	—	—	—
Totals	551	786	202	32	93
Grand total					1,664

TABLE III. Summary of recorded bud mutations in certain fruits and vegetables, most of which have as yet received but little study.

Variant forms	Fruit variations	Foliage variations
Almond	1	1
Apricot	3	5
Banana	12	—
Blackberry and dewberry	2	28
Currant	7	6
Date	3	—
Fig	7	2
Gooseberry	6	1
Jujube	—	1
Olive	1	—
Persimmon	2	—
Pineapple	25	4
Raspberry	3	—
Strawberry	1	1
Walnut	2	—
Irish Potato	91	3
Sweet Potato	15	1
Totals	181	53
Grand total		234

mercial apple varieties as follows: Delicious, 57; Winesap, 29; Rome Beauty, 21; Northern Spy, 17; Oldenburg, 15; McIntosh, 11; Gravenstein, 13; Fameuse, 10; Stayman Winesap, 10; Stark, 7; Esopus Spitzenburg, 5; Jonathan, 5; Twenty Ounce, 5; Baldwin, 4; Red Astrachan, 4; Tompkins King, 4; York Imperial, 4; St. Lawrence, 3; Wealthy, 3; Willowtwig, 3; and one or two in each of the varieties Ben Davis, Bramley, Chenango, Cox Orange, Dougherty, Hyslop, James Grieve, Minkler, Rambo, Ribston, Shiwassee, Summer Queen, Summer Rambo, Transcendent, Wagener, Western Beauty, Westfield, Winter Banana, Wolf River.

In addition to the red sports, many other apple sports have been found such as those that differ from the normal in size, shape, texture of peel, seediness, texture and flavor of flesh, and in quantity of production. These, by reason of the difficulty in measuring them and in distinguishing them from the normal forms, have not been so spectacular as the color-of-fruit sports. However, some of them seem to be as important commercially as the color sports, and evidence is increasing that they may eventually rival the color sports in public interest and in commercial importance.

In pear, peach, plum and cherry varieties a number of strains that originated as bud sports have become commercially important during recent years. Most of these differ from the parent variety in the time of maturity of the fruits, in color, size and shape of fruits, and in size or amount of seeds. Many others are in process of progeny testing both experimentally and commercially. This is also true to some extent of grape, youngberry and some other fruits.

In roses,[20] dahlias,[45] chrysanthemums,[44] ferns[3] and many other flowers and ornamental plants,[32,42,43,66] the list of bud sport varieties now in quite general use is a long and important one, as is well known by all workers with these plants. The possibility that new forms of many flowers and shrubs originate frequently as bud variations and remain constant under commercial propagation has been well known to growers for many years, but even with that knowledge, the possibility that variations of this kind were constantly occurring in fruit varieties was usually overlooked or actually denied until quite recent times. Carrière[6] recorded in 1865 a very incomplete list of 50 standard rose varieties known to have been of bud mutation origin. The 1919 Annual of the American Rose Society listed 429 varieties of American roses of which 116, or more than 27 per cent, were known to have originated as bud mutations.[20] Of 83 roses that were patented before May 12, 1936, 33 originated as bud mutations.[67]

A large proportion of the present cultivated varieties of chrysanthemums are of bud mutation origin. Cramer[11] presented a list of 400 such varieties and it has been stated that of the 8,800 varieties cultivated in Europe in 1899 more than 5,000 originated as bud variants.

With the increasing use of X-rays on buds, it seems almost certain to the writers that the lists of varieties originating from bud sports will be rapidly increased within the relatively near future.[27,65] In general, it seems apparent that most of the combinations of characters brought about through cross-pollination are also likely to be found occurring in bud sports. With our growing knowledge of the use of X-rays and other methods of stimulating somatic variations, it is not without the range of possibility that bud sports may eventually become practically as important a factor in plant improvement as is cross-fertilization in the case of plants that are commonly reproduced sexually.

Systematic Search for Bud Mutations

Through the medium of daily newspapers and agricultural periodicals, the writers have repeatedly suggested to growers and other interested persons the importance of a systematic search

for bud mutations in their orchards and plantations. The abundant response to these requests indicates an active interest in this pleasant work on the part of plant growers. Some of these discoveries are now under commercial test with the promise of rather valuable additions to the varieties of those plants under consideration.

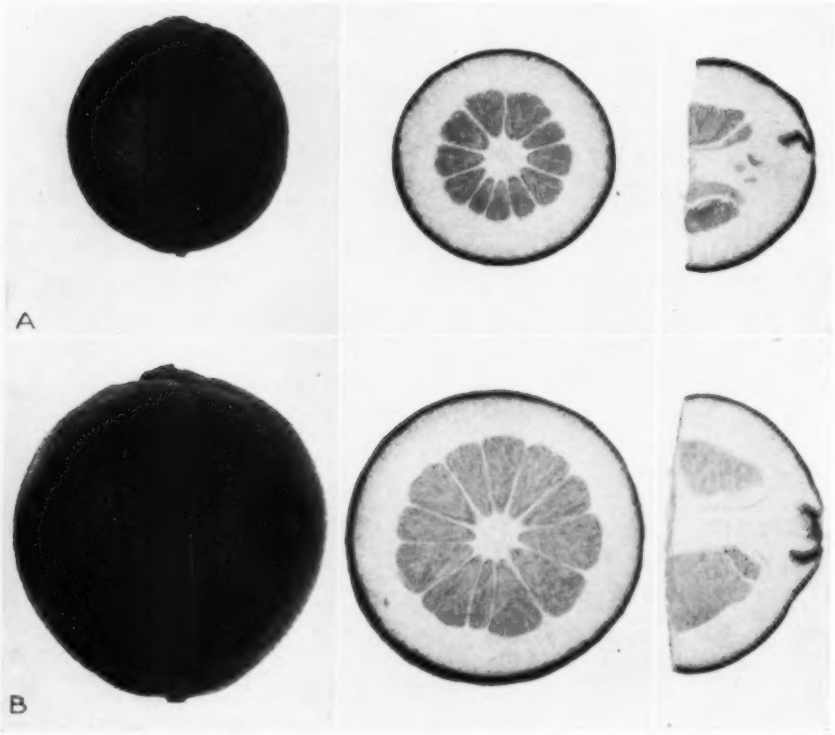
It may be of interest to briefly outline our suggestions which have proved to be most helpful in this search. While these experiences are elementary in nature they have proved to be of more than ordinary value in these pioneer studies as shown by personal contacts and through correspondence with many growers of deciduous, citrus and other crops and ornamental plants.

The most frequent question asked by growers is, "When can one best look for bud mutations?" Our answer to this question has been to look for fruit sports when the fruits are about mature. If early maturing mutations are sought, the trees should be gone over carefully from 2 to 3 weeks before the fruits are ready to be harvested. If late maturing bud mutations are desired, the differences in color of the mature fruits and those that ripen two or three weeks later are usually conspicuous. In several instances the writers have found late maturing limb sports by going over the orchard after the crop has been picked, the immature fruit on the late maturing limbs having been left by the pickers for a later harvest.

The second most frequent question is "How can I look for bud mutations?" Having selected the most appropriate time for the search, the observer should examine each tree or plant in the light of a mental picture of the normal forms. Any marked departure from the normal will then likely catch the attention of the systematic observer. Differences in the color of fruit, flowers, or foliage are likely to first strike one's attention. The shape of the fruits, flowers or foliage is also easily noticed after the typical shapes of the normal forms have clearly been fixed in mind. Texture of the peel of fruits is also rather conspicuous, especially in the citrus and to a lesser extent in some of the deciduous fruits. The time of day during which the search can best be made is of some importance. The writers have found that during the morning hours the conditions are most favorable for detecting the unusual owing to light conditions, the turgidity and freshness of the foliage, and also perhaps because one's powers of observation are keenest during that period of the day. However, to those who live in or near their plantation, it pays to keep the trees or other plants under rather careful observation at all times.

The third question that has reached us most frequently in this connection has been "Where can I best search for bud sports?" An answer to this question is difficult because they may appear anywhere and in plants of all ages. As a rule, in fruits and other plants that the writers have studied, their experience indicates that better results are likely to be obtained with healthy trees grown under favorable environmental and cultural conditions at or near full bearing age, or that are fully developed. It is essential that the search for mutations should cover a considerable period of time so as to allow for rather frequent inspections.

The most striking mutations perhaps are those where one or more limbs or branches of an otherwise normal plant produce fruits, foliage or flowers that are quite distinct from the normal in one or more characteristics—in other words, what are now commonly called "limb sports." In addition, the entire plant may be a sport, the result of the unintentional propagation of a bud from a limb or twig sport. Outstanding examples of both these occurrences include the original red-fruited limb sport in the otherwise normal Delicious apple tree which was the origin of the Starking variety, and the entire-tree Delicious sport from which the Richared variety arose. Bud mutations of the peach and orange that were found as limbs in trees that were otherwise normal are shown in Figures 7 and 8.



EARLY GROWTH-RATE AFFECTED BY BUD MUTATION

Figure 8

Young Robertson Navel oranges (*B*) and normal Washington Navel oranges (*A*), showing comparative early rapid growth of the Robertson fruits. This strain originated from a limb sport in an otherwise normal Washington Naval orange tree.

The desirability of a systematic search for bud mutations is that through trained observation by plant growers and plant lovers, many new plant forms may be discovered and tested with the hope that some of them mav prove to be of commercial value. This search is open to all who may be interested. It is worth while if for no other reason than that of learning more about certain plant characteristics.

Progeny Tests

After observing limb or entire tree or other plant variations it is necessary to ascertain whether or not they are inherent (and capable of perpetuation through bud propagation) or whether they are mere fluctuations that develop as a result of local environmental influences and are not transmitted by propagation. The usual method of determining these points is one that is now commonly called the "progeny test." This term means the propagation through budding of one or more plants of the variations under consideration. With tree fruits there are at least two ways in which this test is accomplished — first, through topworking bearing trees with buds from the variations, and second by budding on seedling rootstocks. Under the first method somewhat quicker fruiting results are to be

obtained than through the budding on seedlings. In general, the topworking method is oftentimes desirable and has the advantages of economy and operation and an early conclusion, but the budding of seedlings is the more satisfactory method, especially when quantitative as well as qualitative characteristics are being studied. While one or more topworked trees will indicate whether or not the shape, color, size and some other characteristics of the fruits, flowers or foliage of the variations are capable of transmission through budding, the writers believe that at least five, preferably more, budded nursery trees should be used for comparison with an equal number of such trees of the same age and conditions of growth that have been propagated from the normal forms in order to obtain dependable information as to quantity of fruit and certain other commercially valuable characteristics. Naturally, the method of carrying out satisfactory progeny tests will depend upon circumstances and will have to be adapted to each case and be governed by local conditions.

Commercial Use

When the progeny tests indicate that the experimental strains may be of potential value, commercial tests covering a reasonably large acreage should be carried out so as to determine definitely the adaptability of the strains for commercial culture, from the producing, distributing and marketing standpoints. Such tests, if adequate and conducted over a period of years, will be dependable guides as to the relative merits of the strains under test as compared with the parent varieties. These progeny and commercial tests will tend to avoid disappointments that oftentimes follow hasty conclusions that are based upon insufficient evidence and immature judgment. While they require considerable time and some systematic effort, the writers believe that the results fully justify the time and expense required in making them. If such tests are not practicable in individual instances it is usually possible to obtain the cooperation of state or other agricultural workers, especially if the indications are that the results are likely to be of some scientific interest or commercial importance. Furthermore, it is desirable to obtain the suggestions and advice of such workers in any contemplated effort along these lines. The established sources of information on these subjects are the Agricultural Experiment stations that are usually located at the State Colleges, and the U. S. Department of Agriculture at Washington, D. C.

Conclusion

The growing scientific interest in the phenomenon of bud mutation is shown in some degrees by the increasing list of publications that present the results of researches along this line. During recent years the rapidly developing commercial interest in this subject is shown by the increasing number of strains of vegetatively propagated horticultural and agricultural crops that are now in commercial use in this country. The widespread activity in the search for promising bud mutations is reflected by the rather rapid increase in the numbers of those reported in recent years which have been summarized in the tables in this article.

There has been some progress made in the study of the stimulation of the frequency of bud mutations in certain plants through irradiation of somatic tissues with X-rays. It seems probable that the exploration of this field for research has just begun and that the future studies along this line will be of great value to some of our horticultural and agricultural industries.

The increasing number of growers, professional horticulturists and scientific workers who are interesting themselves in this subject makes it certain that major advances in our knowledge regarding bud mutation and bud selection still lie ahead. This will be reflected by great advances on the practical side because of the improved strains that will be developed commercially.

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